May 26, 1853.

The EARL OF ROSSE, President, in the Chair.

The following communications were read:—

1. A letter from Mr. Joule to Colonel Sabine. Communicated by Col. Sabine, Treas. V.P.R.S. &c.

Acton Square, Salford, May 23, 1853.

My dear Sir,—I notice in the Proceedings of the Royal Society for April 21, a letter from M. Regnault in which some experiments of my own are referred to in a manner which I feel does me injustice. M. Regnault says, "Le nombre trouvé par M. Joule pour la chaleur spécifique de l'air sous pression constante (0·226) est beaucoup trop faible. Celui qui résulte de mes expériences très nombreuses, et faites dans des circonstances variées, afin de reconnaître et d'éliminer les erreurs constantes, est 0·237."

Now, in my paper on the air-engine, Phil. Trans. 1852, part i. p. 74, I have given the results of three series of experiments, viz. 0·23008, 0·22674, and 0·2325, and remark, "The mean of the three results is 0·22977, or nearly 0·23, which we may take as the specific heat of air at constant pressure determined by the above experiments."

I had been informed that M. Regnault was working on the specific heat of gases, and on that account did not feel it desirable to enter upon the laborious investigation which would have been requisite in order to add a couple of decimal figures to the number I had arrived at, and which was sufficient for the object I had in view, viz. to show that the discrepancy between the actual and theoretical velocity of sound arose from the incorrectness of Delaroche and Berard's determination of the specific heat of air (2·67), and not from any notable error in my number for the mechanical equivalent of the thermal unit. Having succeeded in doing this, I calculated the Tables 3 and 4 of my paper, using 0·238944 for the specific heat of air under constant pressure. I feel much gratified that the result arrived at by so eminent an experimentalist as M. Regnault confirms the accuracy in the main of the number I adopted.

I have only to add that Professor Thomson and myself, in pursuing our research on the thermal effects of rushing elastic fluids, are following up the views on the relation between mechanical and thermal phenomena originated by ourselves; and we shall feel most happy if M. Regnault's results, in the important line of investigation he has adopted, will facilitate our labour.

I have the honour to remain, dear Sir,

Yours most truly,

Colonel Sabine, &c. &c. &c. J. P. Joule.

2. "Experimental Researches on Vegetation." By M. Georges Ville. Communicated by The Earl of Rosse, P.R.S. Received May 26.

After stating that it has often been asked if air, and especially

azote, contributes to the nutrition of plants; and, as regards the latter, that this question has always been answered negatively, the author remarks it is however known that plants do not draw all their azote from the soil, the crops produced every year in manured land giving a greater proportion of azote than is contained in the soil itself. The question which he has proposed to himself for solution is, whence then comes the excess of azote which the crops contain, and in a more general manner, the azote of plants, which the soil has not furnished? He divides his inquiry into the three following parts:—

First. Inquiry into and determination of the proportion of the ammonia contained in the air of the atmosphere.

Second. Is the azote of the air absorbed by plants?

Third. Influence on vegetation of ammonia added to the air.

1. The author remarks that since the observation of M. Théodore de Saussure, that the air is mixed with ammoniacal vapours, three attempts have been made to determine the proportion of ammonia in the air: a million of kilogrammes of the air, according to M. Gräyer, contain 0·333 kil. $A2H^3$; according to Mr. Kemp 3·880 kil.; according to M. Frésenius, of the air of the day, 0·098 kil., and of night air, 0·169 kil. He states that he has shown the cause of these discrepancies, and proved that the quantity of ammonia contained in the air is 22·417 grms. for a million of kilogrammes of the air; and that the quantity oscillates between 17·14 grms. and 29·43 grms.

2. The author states that though the azote of the air is absorbed by plants, the ammonia of the air contributes nothing to this absorption. Not that ammonia is not an auxiliary of vegetation, but the air contains scarcely 0·0000000224, and in this proportion its effects are inappreciable. These conclusions are founded upon a great number of experiments in which the plants lived at the expense of the air without deriving any thing from the soil. For the present he confines himself to laying down these two conclusions:—1. The azote of the air is absorbed by plants, by the cereals, as by all others. 2. The ammonia of the atmosphere performs no appreciable part in the life of plants, when vegetation takes place in a limited atmosphere. After describing the apparatus by means of which he carried on his experiments on the vegetation of plants placed in a soil deprived of organic matter, and the manner in which the experiments were conducted, he adduces the results of these experiments in proof of the above conclusions.

3. With reference to the influence of ammonia on vegetation, the author states that, if ammonia be added to the air, vegetation becomes remarkably active. In the proportion of 4 ten-thousandths the influence of this gas shows itself at the end of eight or ten days, and from this time it manifests itself with a continually increasing intensity. The leaves, which at first were of a pale-green, assume a deeper and deeper tint, and for a time become almost black; their petals are long and upright, and their surface wide and shining. In

short, when vegetation has arrived at its proper period the crop is found far beyond that of the same plants grown in pure air; and, weight for weight, they contain twice as much azote. Besides these general effects there are others which are more variable, which depend upon particular conditions, but which are equally worthy of interest. In fact, by means of ammonia we can not only stimulate vegetation, but, further, we can modify its course, delay the action of certain functions, or enlarge the development and the modification of certain organs. The author further remarks, that if its use be ill-directed, it may cause accidents. Those which have occurred in the course of his experiments appear to him to throw an unexpected light upon the mechanism of the nutrition of plants. They have at least taught him at the expense of what care ammonia may become an auxiliary of vegetation. These experiments, which were made under the same conditions as those upon the absorption of azote, are then described, and their numerical results given.

To the conclusions already stated, the author adds that there are periods to be selected for the employment of ammonia, during which this gas produces different effects. If we commence its use when several months intervene before the flowering season of the plants, it produces no disturbance; they follow the ordinary course of their vegetation. If its use be commenced at the time of flowering, this function is stopped or delayed. The plant covers itself with leaves, and if the flowering takes place all the flowers are barren.

3. "An Account of Meteorological Observations in four Balloon Ascents made under the direction of the Kew Observatory Committee of the British Association." By John Welsh, Esq. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S., President of the British Association, on the part of the Council of the Association. Received April 27th, 1853.

The object contemplated by the Kew Committee in the balloon ascents, of which an account is given in this communication, was chiefly the investigation of the variations of temperature and humidity due to elevation above the earth's surface. Specimens of the air at different heights were also obtained for analysis.

The instruments employed were the barometer, dry- and wet-bulb hygrometer, and Regnault's condensing hygrometer.

The barometer was a siphon, on Gay-Lussac's construction, without verniers; the upper branch of the siphon being alone observed, corrections having been previously determined for inequality of the tube at different heights of the mercury.

Two pairs of dry and wet thermometers were used, one pair having their bulbs protected from radiation by double conical shades open at top and bottom for the circulation of the air, the surfaces being of polished silver. The second pair were so arranged, that by means of an "aspirator," a current of air was made to pass over the bulbs more rapid than they would be exposed to by the mere vertical motion of the balloon. The object of this arrangement was to